COLONEL BASEVI's criticisms of Clausius' virial theorem are not justifiable.

In the first place, the left-hand side of his equation at the foot of p. 413 should be $ux - [ux]_{t=0}$, since the latter term is not necessarily zero even for periodic motion; *e.g.* it equals 1 if $x = \exp. (\sin t)$.

In the next place, though this difference obviously vanishes for periodic motion when the "suitable value given to t" is a multiple of the period, yet for this *same* value of t the areas $\int udx$ and $\int xdu$ will *not* vanish; indeed for *no* value of t can the former vanish, as it represents $\int (dx/dt)^2 dt$, which is the sum of essentially positive quantities. Hence we can have but $\int udx = -\int xdu$ when $xu = [xu]_{t=0}$.

Thirdly, though in the case of stationary motion the areas $\int udx$ and $-\int xdu$ may not be exactly equal for any value of t, yet their difference can only fluctuate within certain narrow limits, so that when multiplied by $m/2t$ it becomes vanishingly small if t is large enough, which is all that Clausius asserts.

Fourthly, Clausius does *not* take $m\int udx$ to represent kinetic energy, but this expression divided by $2t$.

Fifthly, the fact is overlooked that R refers, not to single particles, but to *pairs* of particles; so that in Lord Rayleigh's case, $\frac{1}{2}\Sigma Rr = \frac{1}{2}Rr$, and not Rr, as asserted, there being only *one* pair of particles in question, and the virial equation *does* therefore give $R = mv^2/\rho$, the ordinary law of force for uniform circular motion.

Lastly, there is no ground whatever for taking $\frac{3}{2}Vr$ and $\frac{1}{2}\Sigma Rr$ as equal terms, there being absolutely no connection between them except that both represent energy; indeed, by this assumption Colonel Basevi obtains a formula which gives for the pressure in an ideal gas only half its proper value.

Christ Church, Oxford. ROBERT E. BAYNES.

Hutton's "Theory of the Earth."

IT is to be doubted whether any work, with the exception of Lyell's "Principles," has had a more important influence on the science of geology than Hutton's "Theory of the Earth," in which for the first time the true mode of studying the science was set forth and its fundamental facts outlined.

The theory was first propounded in a paper of some ninety-five pages, written in 1785, which appeared in 1788 in the first volume of the *Transactions* of the Royal Society of Edinburgh, and was at once attacked by a number of hostile critics.

Ten years later, in 1795, it was republished in Edinburgh, greatly extended, and including the results of much additional work, in two good-sized octavo volumes. These included the substance of a number of papers published by Hutton after the appearance of the first outline, as well as answers to his various critics, and is the work which has become a classic in the science.

The work, however, in its published form is evidently incomplete, for on the title-page it is stated to consist of four parts, and, in the table of contents, volume i. is called part i. and volume ii. part ii. Volume ii., furthermore, concludes abruptly with the following words: "Therefore in pursuing this object, I am next to examine facts, with regard to the mineral part of the theory. . . and endeavour to answer objections or solve difficulties which may naturally occur from the consideration of particular appearances."

Parts iii. and iv., so far as I can ascertain, if written, were never published. In the library of the Geological Society of London, however, there is a manuscript of Hutton's which is apparently a portion of one or other of these parts. It is bound in book form, and was presented to the library by Leonhard Horner, Esq., and in a note by that gentleman, presenting it to the Society, it is stated to be one of a series, and to have been given by Dr. Playfair, the populariser of Hutton's work, to Lord Webb Seymour, and on the death of this nobleman to have passed to the Duke of Somerset, who gave it to Mr. Horner. It bears no title, and consists of six chapters numbered from iv. to ix., and was evidently continued in another manuscript, as the last page, forming the conclusion of chapter ix., bears the words "chapter x." at the lower corner.

The manuscript treats chiefly of a subject the investigation of which has been so prolific of results in recent years, namely granite contacts, and especially the contact of granite masses with "schistus." He shows that the granite was not a "primitive" rock on which the schist was deposited, but that it was intruded through the latter in a molten condition, and holds that it was the agent by which mountain-chains were upheaved, supporting his proposition by a description of the relations of these rocks in various parts of Scotland and elsewhere, among them the Island of Arran. To the description of this last-mentioned locality a whole chapter is devoted, in which the true nature of the pitch-stones is also set forth, and the derivation of the felsites from them by a process of dentrification is recognised.

It is merely desired in the present letter to draw attention to the fact that at least some other volumes of this manuscript are extant, and to urge upon those who may know where they might be sought, or who may by chance come upon them, the importance of preserving them, and of placing them, if not in the Geological Society's library with the fragment above referred to, at least in some library where they may be at once secure and available for use.

The book is one of the most remarkable which has appeared in the history of geological science, and all who are interested in the science must desire to see it secured and preserved in its completed form. FRANK D. ADAMS.

McGill University, Montreal.

Abnormal Atlantic Waves.

IT happens that I have only quite lately seen a letter on this subject in NATURE of March 7, from Mr. E. C. Stromeyer of Glasgow. It may perhaps be of interest to some of your readers to learn that on January 6, 1891, and about 4 p.m., the people of Funchal, the chief town of Madeira, and situate on the south coast (lat. 32° 37′ 45″ N., long. 16° 55′ 20″ W.) were astonished by the arrival of a great wave which burst with violence on the shore, coming seemingly from the S.E. or E.S.E. The sea had been calm previously, and the wind was light. At Machico, a village some fifteen miles to the east of Funchal, a similar phenomenon took place contemporaneously, and also at Camara de Lobos, a village about six miles to the west. At the latter place, where there is a small bay amongst the rocks, there were three risings of the sea, one much higher than the others. The bottom of the bay was laid bare, and fishes were seen struggling in the mud. The boats lying on the beach were more or less damaged, but I did not hear that other property was injured.

Two electric cables belonging to the Brazilian Submarine Telegraph Company connect Funchal with Lisbon. Now, it is worth noting that early on the morning after the occurrence of the great wave, when the Company's officials stationed at Funchal went as usual to test the cables, one of them was found to be broken in deep water at a distance of seventeen or eighteen miles to the south of Madeira, whilst the other cable was in good working order. It is an unsolved question whether the same cause that produced the great wave had also broken the cable, or whether the two events were simply coincident but due to independent causes.

Slight shocks of earthquake are felt at distant intervals at Madeira, but no seismic disturbance was noticed near the date of the great wave.

As the wave came from the southwards, I asked a friend to make inquiry at Tenerife whether anything of the sort had been experienced there. The reply was that nothing extraordinary had occurred on January 6 at Santa Cruz on the south coast of that island. At Puerto Orotava, on the north coast, there was bright weather at the time, with light winds, and no wave had occurred, nor had any earthquake been felt.

JAMES YATE JOHNSON.

Funchal, Madeira, September 17.

Leaf-absorption.

A FEW weeks ago I threw some cuttings of the common Privet (*Ligustrum vulgare*) on the borders in the garden. Of these cuttings some perished, while the remainder were drawn into the soil by the worms, some with the cut end downwards, some only by a single leaf, leaving all the rest of the cutting *en*

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plein air. These latter are, at the end of this time, all as fresh and healthily green as they were at the moment of cutting off the parent plant, notwithstanding the very hot weather we have recently experienced. It seems to me that this is a clear proof that the *rôle* of leaves is to *absorb* as well as *evaporate*, a point on which much doubt has often been expressed. G. Paul.

Harrogate, September 29.

It has been proved over and over again, and it is easy to prove, that the leaves of some plants, though probably not of all, are capable, under certain conditions—usually abnormal conditions—of absorbing aqueous vapour or fluid water; but this action can hardly be regarded as a function, though I am not prepared to say that absorption of moisture by leaves is in no case a part of the every-day life of a plant. The return to turgidity of the leaves of a plant during the night is, however, in a general way, due, doubtless, to reduced transpiration, rather than absorption from the atmosphere. Yet in the absence of a counteracting current of water from the roots, the leaves of some plants, especially of those inhabiting almost rainless regions, but where the air sometimes reaches almost complete saturation, absorb moisture. At least, so it is asserted. With regard to absorption by detached leaves, or by leaves of detached branches, the development of the action depends, apart from other circumstances, on the amount of vital energy left; and this is determined, to a great extent, by age. The common Privet is a shrub of extraordinary vitality, rare in our native vegetation. I hardly need add that proof of leaves being able to absorb water may be had by inserting withered leaves in water by their upper halves, leaving the stalk out. If not too old, or too much dried, the whole leaves will regain turgidity, though the process may be a slow one.

W. Botting Hemsley.

Tertiary Fossil Ants in the Isle of Wight.

In a paper published in Nature for August 22, p. 399, by Prof. C. Emery, on "The Origin of European and North American Ants," the author states that "the Sicilian amber of Miocene age contains genera which belong to the actual Indian and Australian fauna, while the Baltic amber contains the genera Formica, Lasius, and Myrmica." In the Bembridge limestone in the Isle of Wight, of Eocene age, the same as the Baltic amber, the following genera occur: Formica, Myrmica, and Camponotus, and some others not yet described. In my collection there are a large number of these Hymenoptera, generally well preserved, and seem to be more numerous than any of the other insects from the same beds. It is only of late years that any number of insects have been met with in the British tertiaries, and it is well to record the two genera referred to Formica and Myrmica, being found both in the Baltic amber and Bembridge limestone. Among the numerous fossil insects in my possession from the Lias, no trace of any ants has been observed, and it seems that they did not come into existence until the later Tertiary epoch. P. B. Brodie.

THE NORMAL SCHOOL AT PARIS.

In connection with the celebrations of the centenary of the foundation of the École Normale in Paris, referred to at the time in these columns (vol. li. p. 613), a ponderous tome has been published[1] containing the complete history of the school, and details concerning the most renowned of its alumni. Opportunity is thus afforded of giving a sketch of the development of a school which has played an important part in the history of education for nearly a century, and which has been the training college of many of the most distinguished Professors in France.

The most elaborate article in the volume is a detailed history, by M. Paul Dupuy, on "L'École Normale de l'an III." That article has furnished the particulars with reference to the early history of the school given in this contribution.

When the Convention of the 9th Brumaire, An. III. (October 30, 1794), passed the decree to which the Normal School owes its foundation, it realised an idea which had occupied the attention of the University and Parliament for many years. So far back as 1645 the University of Paris considered a proposal by the rector, Dumonstier, to provide the means for the education of teachers and principals. After the expulsion of the Jesuits in 1761, the Parliament of Paris began to carry out the idea by instituting fellowships and uniting at Louis-le-Grand the scholars of the small colleges of the University. At the time when Parliament was taking these steps, Barletti de Saint-Paul was forming a training school for teachers, in which his principles of personal pedagogy were taught; and Bernardin de Saint-Pierre pleaded for a college of instruction. "J'admire avec étonnement," he wrote in 1789, "que tous les arts ont parmi nous leur apprentissage, excepté le plus difficile de tous, celui de former les hommes." To the influence which these educational reformers had in bringing the matter before the Government of the Revolution must be added the impulse derived from Germany, through Alsace. Alsace was then the only province of France able to furnish ideas and models for popular instruction. It had been touched by the great pedagogic movement in Germany, and its great influence upon the three Revolutionary Assemblies makes it prominent in the history of the Normal School.

Practically every part of the educational system of France owes its development to the Republic. The Committee of Public Welfare early concerned itself with the question of national education, and Commissions were appointed to report upon the best means for developing an educational system. In 1793 a plan was put forward to establish normal schools for the training of teachers. Nothing definite was formulated, however, with reference to the Normal School until September 1794, when the Committee of Public Instruction adopted a series of articles, the first of which was to the effect that "there should be established, at Paris, a Normal School, where instruction in the art of teaching science should be given to persons already possessing scientific knowledge." At the end of the following month, the National Convention, after a discussion of the scheme and the subjects to be taught, passed a law for the establishment of Écoles normales. The idea was to establish these schools in various parts of France, but it was not then realised, and the Normal School at Paris is the only one that owes its existence directly to the law of the Convention. Referring to the designation of the schools, an official note reads: "The word *normal*, which has been applied to the schools newly decreed, is taken from geometry. It expresses really the perpendicular or level. In the sense employed in this case it announces that all knowledge belonging to science, to the arts, to belles-lettres, &c., will there be taught, and taught to all equally." Science was thus placed upon the same footing as the humanities. The methods and results of investigation were not to be known to a few, but were to be taught by the most eminent men it was possible to obtain. The first programme of the courses and professors shows the scope of the instruction given.

Subjects.	Professors.
Mathematics	Lagrange and Laplace.
Physics	Haüy.
Descriptive Geometry	Monge.
Natural History	Daubenton.
Chemistry	Berthollet.
Agriculture	Thouin.
Geography	Buache and Mentelle.
History	Volney.
Morals	Bernardin de St. Pierre.
Grammar	Sicard.
Analysis of the Understanding ...	Garat.
Literature	La Harpe.

A glance at this list will show that the professors were selected on account of their eminence in different

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[1] "Le Centenaire de l'Ecole Normale." (Paris: Hachette et Cie.)

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